joints the same as in gout. Thus, in the knee, the typical points of tenderness are on the tendon of the quadriceps just above the patella, and below it also at the tendon insertion. Posteriorly equally characteristic points of tenderness are found along the tendons of the hamstring muscles. Out of 58 cases of inflamed knees in my list, 53, or 91.4 per cent., were thus typical, and in 5, or 8.6 per cent., the pain was uniformly diffused over the joint. In no case were the condyles the seat of most tenderness.

Rheumatic inflammation of the ankles is not so distinctive in the location of painful points as in the case of the knees. Of 32 cases my notes are that the most pain was elicited by pressure on the tendons of the dorsal aspect of the joint in 20, or 64.4 per cent. This pain, however, is not so pronounced as in the tendons of the quadriceps and hamstring muscles of the knees. In 3 cases, or 10.6 per cent., the most painful points were under the malleoli; and in 8, or 25 per cent., there was no specialized place.

The hips were affected four times, but no localized pain was noted in either.

Of the joints of the upper extremity, the shoulders were affected in 20 cases. In only one of these was a special tender point found on the coracoid process.

The elbow was affected in 13 cases. Of these in 9, or 70 per cent., the most painful point was elicited by pressure on the triceps tendon. In 4, or 30 per cent., there were no special points.

The wrists were affected in 24 cases. Of these the most pain was elicited by pressure on the tendons of the dorsal and palmar aspects in 20, or 83.3 per cent. It was uniformly diffused in 3, or 15 per cent.; and one case complained of most pain on transverse pressure, as in gout.

In every case noted of inflammation of the finger-joints in the rheumatic patients the pain was localized in the tendons and not on the condyles.

Why the hips and shoulders do not commonly show specially painful points may be due to these joints being so covered by large muscles.

It seems natural to infer that the difference between these two forms of arthritis in the respects noted, of pain on pressure, is because gout chiefly affects the periosteal, while rheumatism chiefly attacks the synovial structures.

With the exception of the hips and shoulders, therefore, it would seem that the distribution of pain on pressure may be often of use in the discrimination of these two articular inflammations. The ankles and wrists also do not present such marked contrasts as the more simple joints, apparently from the greater number of bones entering into the formation of these articulations.

impossible to admit that the nerve-cells of the inferior centres possess a direct importance either in relation to reflex movements or to the voluntary movements, and that this seems to apply equally well to the nerve-cells of the superior centres. This view forces us to the conclusion that the activity of the nervous system, intelligence, consciousness, etc., is seated really in a fibrillary meshwork of the cortex, the molecular layer, and has nothing to do with the nerve-cells of the cortex. The latter, indeed, deprived of their psychic functions, become simply "trophic" centres. They serve only for the maintaining of the nutrition of the nerve-fibres and their innumerable arborescent ramifications.

This view of Nansen's has been adopted by my esteemed colleague, Dr. Mills, who, in a discussion on cerebral localization at the last meeting of the American Neurological Association, and also in the *Text-book on Nervous Diseases by American Authors*, has maintained this position; he holds that "impulses are conveyed from processes to processes through the entire reflex arc, through the entire length of a cortico-efferent, or a cortico-afferent, projection-system," without passing through nerve-cells, and that "the function of the nerve-cell body is trophic;" that its "nuclei and nucleoli preside over the nutrition of the long or short fibres which pass out of or grow into them;" and, further, that cells are of enormous bulk in order that they may be able to sustain these processes. In his words, "the aggregations of gray matter at various levels of the nervous system are watering- and feeding-places, not places for renewing nerve-activity."

Certainly this view does violence to the fundamental principle that the properties of a given tissue depend upon its cells, and that the cells are the integral parts of the tissues. Nowhere in the whole range of biology do we find a similar anomaly obtain as is implied by this view. It asserts that merely incidental structural attributes are of greater value than the individual cells, whose building up constitutes the tissue. Surely there is here an intrinsic contradiction.

Not only upon general principles is this view untenable, subversive of all that we have hitherto learned; but, if carefully analyzed, it is found to present insuperable difficulties. If it be true that a nerve-fibre diffuses the energy which it conveys: in a general way, scatters it though all the fibres or nerve-cell processes near which it happens to lie, it becomes impossible to explain the definite and precise actions of the nervous system, properties which are so characteristic of it. Nothing but hopeless confusion of function could result if such a thing were possible. It would mean that nerve-currents course indiscriminately without relation to each other through this network of fibres. It would mean that everything that had been done by nature to conserve and isolate nervous impulses by enclosing the nerve-fibres in special sheaths of insulating material previous to their entrance into the cortex had, after all, no purpose,

ON THE FUNCTIONS OF THE NEURON,

WITH ESPECIAL REFERENCE TO THE PHENOMENA PRESENTED BY HYSTERIA AND HYPNOTISM.

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THERE has been noticeable of late years a tendency strangely to misconstrue the structure of the nervous system and to misinterpret the truths which that structure teaches. Curiously enough, this tendency has appeared with our increasing knowledge of the details of the intimate structure of the neurons and of the relations which they bear to each other. It is known, for instance, that the afferent fibres, those which convey impressions from without up to the cortex [themselves processes (axons) of peripheral neurons], terminate not in nerve-cells, but in the uppermost layer, the so-called molecular layer or molecular plexus of the cortex. Here in some way the minute divisions of the afferent fibres affect the fibres of this molecular plexus. The latter are in turn in direct communication with the nerve-cells of the cortex, are, in fact, themselves cell-processes. These facts suggested the idea that afferent impulses are diffused through the molecular plexus without involving or affecting the nerve-cells at all, and according to this interpretation the nerve-cells are left out of consideration entirely in studying nervous phenomena. This view has been especially advanced by Nansen, who maintains that the old manner of view relative to the composition of the reflex arc and the physiological importance of the nerve-cells can no longer be sustained, inasmuch as the cells are not in direct communication with each other, and because direct communication between the central nerve-cells and the sensory or centripetal nerve-fibres is equally lacking. The reflex arc is constituted, according to Nansen,[1] first, by the centripetal nerve and its fibrillary ramifications passing directly into the nervous meshwork of the cortex—that is, into the molecular layer; secondly, by the propagation of the excitation through this molecular plexus; thirdly, by the transmission of stimuli to the minute lateral branches of the centrifugal or motor nerve-fibres. It follows that impulses are transmitted to the superior centres without passing directly through the nerve-cells. "We can admit in the same manner," continues Nansen, "that the voluntary impulses emanating from the nerve-fibres which emerge from the superior centres transmit themselves directly to the centrifugal fibres of the inferior centres without passing through the nerve-cells of these centres." He says, further, that it is cells save that of propinquity or perhaps contact. The individuality of the nerve-cell as a morphological integer is wholly preserved.

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[1] Soury, Les Fonctions du Cerveau, p. 316.

If we grasp this idea in its full meaning, our conception of the nervous system changes profoundly. It is no longer a stringing together of telegraph wires and way stations, but it consists of an aggregation of cell-integers, each one of which does its share in the production and in the transmission of nervous energy. For instance, the impulse proceeding from a motor neuron in the cortex is transmitted by the neuron through its own protoplasmic extension (the efferent nerve-fibre) to a definite aggregation of cells in the spinal cord. It communicates its energy to these cells in the spinal cord without in any way fusing with their protoplasm or their processes. In the same way the impressions that come by the various sensory paths come from peripheral neurons, those situated in the skin, in the retina, in the ear, in the taste-buds, or in the Schneiderian membrane, and are conveyed by fibres which are merely protoplasmic extensions of these peripheral neurons up to the cortex. Here in turn these fibres transmit the energy they convey to the cortical neurons without fusing with the latter or with their processes. Everywhere, and no matter in what light we view the nervous system, the signal importance of the nerve-cell as an individual entity is strikingly apparent.

A consideration of the above facts has suggested to me the following thought: Can it be that the neuron is not an absolutely fixed morphological element? Can it be that it possesses a certain, though perhaps limited, power of movement? Realizing the practical value and the wide application of this idea, I have examined the literature to see whether a similar interpretation of nervous phenomena has occurred to others, and to gather such facts, if any, as could be brought forward in its support. I found that this thought had occurred independently to three observers, one in Germany and two in France. I found that in 1890 Rabl-Rückhard, in a short paper published in the *Neurologische Centralblatt*, had suggested the view that nerve-cells have an amœboid movement, and he also hinted briefly at the possible significance of such a fact, if true, upon our interpretation of the phenomena of hysteria. Rabl-Rückhard's ideas attracted little or no attention, and in August, 1894, without any knowledge of Rabl-Rückhard's theory, in a paper on "A Case of Hysteria of Peculiar Form," published in the *Revue de Médecine*, Lepine advanced the same view. In endeavoring to interpret the various shifting phenomena observed in his patient he advanced the idea that the neurons were capable of movement to such an extent as to enable them to vary the degree of their relation to each other. About half a year later Mathias Duval, without any knowledge of either the views of Rabl-Rückhard or those of Lepine, in a communication made to the Société de Biologie, advanced the same theory. A week later Lepine, before the same body, repeated his former arguments in its support.

Curiously enough, this view, so suggestive, so pregnant with possibilities, did not meet with the indorsement either of that veteran histologist, Kölliker, or that other high authority, Ramón Cajal. In a paper entitled "Some Conjectures on the Anatomical Mechanism of Ideation, Association, and Attention," published in the *Revista de Medicina y Cirurgia Practicas,* May 9, 1895, Ramón Cajal contended that the nerve-cells do not move, because (1) the terminal branches of the nerve-cells of the cerebellum, of the olfactory bulb, of the acoustic ganglia, optic lobes, etc., always present the same shape and the same degree of approximation to the cell-bodies irrespective of the mode of death of the animal (chloroform, hemorrhage, curare, strychnine, etc.); (2) because the terminal nerve-branches of the retina and of the optic lobes in reptiles and bactrachians presented always the same appearance, no matter whether the organs mentioned had remained in a condition of rest (the animals having been killed after remaining in darkness for a long time) or whether they had been functionally active (the animals being killed after prolonged exposure to sunlight).

While Ramón Cajal thus opposes the theory of mobility of the neuron, he maintains, on the other hand, that the neuroglia-cells possess a great degree of mobility. He points out, for instance, that the neuroglia-cells of the cortex are at times stellate and at others much elongated. Their processes have numerous short, arborescent, and plumed collaterals. Two phases can be observed in them: first, a stage of contraction, in which the cell-body becomes augmented while the processes become shortened and the secondary branches disappear; secondly, a stage of relaxation, during which the processes of the neuroglia-cells are again elongated. Ramón Cajal maintains that the processes of the neuroglia-cells in reality represent an insulating or non-conducting material, and that during the period of relaxation they penetrate between the arborizations of the nerve-cells and their protoplasmic processes, and render difficult or impossible the passage of nerve-currents. On the other hand, when the processes of the neuroglia-cells are retracted, the various nerve-cell processes which they formerly separated from each other are now permitted to come into contact. To me it seems as though Ramón Cajal admits the very thing against which he contends. Evidently, if the nerve-cell processes are not at one time in contact, and at another are in contact, they must certainly move, and the question at issue is self-admitted It certainly does not matter whether the nerve-cell processes move little or move much, but that they move at all is the question at issue; and this, it seems to me, Ramón Cajal admits, although he makes that movement a purely passive one and dependent upon the interposition of the processes of the neuroglia-corpuscles. It is certainly a minor point whether the movement of the nerve-cell processes is active or passive, though it seems that it is far from evident, from the histological facts at our disposal, that the neuroglia-corpuscles play the *rôle* of an insulating material. To me it further seems that a single positive observation outweighs all other negative observations, no matter how great the authority behind them, and this positive observation has actually been made. Wiedersheim[1] actually saw in the living animal, *leptodora hyalina*, an entomostracan, the nerve-cells in the œsophageal ganglion move. The œsophageal ganglion may in one sense be regarded as the brain of the animal, inasmuch as it receives the fibres of the optic nerve and Wiedersheim saw its cells move and change their shape. He describes their movement as slow and flowing. Certainly this observation possesses a profound significance. Even if the animal in which the phenomena were observed is far removed from the vertebrates, it must be remembered that it is just in the lower forms that general biological truths must be sought for, and it is just in the lower forms that they have been found. I do not for a moment contend that the nerve cells of vertebrates possess a gross amœboid movement as in the œsophageal ganglion of the entomostraca; but I do contend that it is in the highest degree probable that such facts as we have, scanty though they be, are in favor of the view that a certain amount of movement does take place in the terminal portions of their processes, their dentrites and their neuraxons, although this movement is probably small in extent.

Let us turn our attention for a moment to the subject of hysteria, and let us see what a flood of light is cast upon this subject, heretofore so obscure and mysterious. Let us take the simple example of a hysterical paralysis, and see how easily it is explained. The neurons of a certain area of the cortex, for instance, retract the terminal branches of the neuraxon to such an extent that the latter are no longer in contact, or sufficiently near to the neurons of the spinal cord which supply the muscles of the paralyzed part. It explains also the marvellous fact that a hysterical paralysis may at one time be so real, so genuine, as to be indistinguishable from a grossly organic paralysis, and yet the next moment upon a suggestion may absolutely disappear. The shifting of symptoms in hysteria, this sudden disappearance of paralysis or anæsthesia, can be explained by the view here advanced as it can be by no other. When power is suddenly re-established in a hysterically palsied limb it simply means that the terminal branches of the cortical neuraxon, previously retracted, are again extended so as to re-establish the proper relations with the spinal neurons. Take again the example of a hysterical anæsthesia. How often do we see a segmented anæsthesia or a hemi-anæsthesia coming and going under the influence of no other stimulus than that which applies to the psychic make-up of the individual, namely, a treatment which we call mental or moral treatment, or that more powerful treatment, suggestion under hypnotism. It would be interesting, indeed, to follow out the ideas here brought forward in their application to the various phenomena presented by hysteria, its sensory, motor, and visceral stigmata. Even the hysterical convulsion, I contend, can be explained by the view here advanced. Time will not, however, permit more than to indicate the line of thought.

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[1] Anatomischer Anzeiger, 1890, p. 673.

When we turn to hypnotism we can see what a ready explanation it affords for the phenomena presented. Under the fixed stare necessitated by the ordinary method of bringing about hypnosis, and under the suggestion of sleep, the neurons are thrown into certain fixed relations with each other, corresponding solely to the ocular strain and singleness of thought induced. At the same time such relations of the neurons as ordinarily bring them into true contact with the outer world are suspended, probably by retraction of cell-processes. We can easily understand, in the light of the theory here advanced, how under hypnotic suggestion a hysterical paralysis disappears, or how, under hypnotic suggestion, anæsthesia is produced in this or that part of the body. Further, the various stages of hypnotism itself—lethargy, catalepsy, somnambulism—are all of them capable of a scientific explanation upon this theory. Hypnotic lethargy, for instance, a stage so easily produced in the majority of patients, merely signifies that so general has been the retraction of the cortical neurons from each other that not only is sleep produced, but also a stage of general motor relaxation, due to the retraction of the terminal branches of the neuraxons in the spinal cord. In hypnotic catalepsy, on the other hand, the reverse obtains. Here the relations of the cortical neurons to the spinal neurons (contact or increased proximity, whatever it may be) are established to a degree beyond that which is normal, and the consequence is an enormous general increase of muscle-tonus. In somnambulism, again, certain of the neurons, especially those which stand in direct relation with the various sensory organs, form partial combinations with a limited number of other cortical neurons, so as to produce the various limited psychic phenomena characteristic of somnambulism, whilst the great bulk of the neurons of the cortex, the summation of whose action constitutes the ego and brings it into close relation with the outer world, have their processes retracted in sleep.

Leaving this interesting field, let us see for a moment of what enormous value this interpretation of cortical action is for normal mental phenomena. Sleep, instead of resulting from brain anæmia, or some other apocryphal condition of the circulation, merely means that when the substance of the cortical cells has been diminished by functional activity, which diminution we have reason to infer, from the researches of Hodge,[1] (on the changes in nerve-cells in fatigue), there comes a time when the cell-processes are retracted, so that the neurons no longer stand in active relation to each other. Interchange of action cannot then take place; unconsciousness follows; sleep is established. Spontaneous wakening merely means that after nutrition has reached a certain point, a point where the wasted cell has been replenished, extension of the cell-processes again takes place, and interchange of active functional relations is re-established.

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[1] Journal of Morphology, 1892, vol. vii. p. 95.

Numerous other ideas also suggest themselves in relation with the view here advanced. Take, for instance, a train of thought. This appears to follow *purely mechanical* lines. Thus a sequence of sound-vibrations impinging upon the peripheral auditory neurons, the auditory cells, produces in them a change, which in turn affects the relations which their neuraxons bear to the auditory nuclei, and secondarily to the auditory cortical neurons. Not only are the latter affected by the impressions received from the afferent neuraxons, but they in turn react in such a way as to change their relations to each other, and the new positions assumed by them will depend largely upon the fact as to whether a similar sequence of impressions has passed through them before. If so, the old combinations will be reformed, and as a corollary the recognition by the ego of the sounds as something heard before. From the cortical auditory centre there now passes through the general cortex a series of combinations among the neurons, also along the oldest and best-travelled lines, so that a given sequence of musical sounds may suggest at first a familiar air, and a moment later a vivid recollection of an opera once heard and seen. Here is the physiology of memory.

A startling thought which also suggests itself in connection with the theory of cortical action advocated in this paper is that *not all of the neurons are functionally active at the same time.* It is exceedingly probable that the number varies greatly. For instance, in fatigued states fewer combinations are made—fewer are possible—because of the exhausted condition, the diminished volume of the cell-protoplasm. After rest, on the other hand, not only are combinations more readily made, but a larger number of neurons are active. It is further probable that in states of mental or emotional excitement the number of neurons that are active and the number of combinations accordingly formed enormously exceed those occurring during ordinary mental states.

Many other thoughts also suggest themselves, and some are extremely interesting. For instance, the receptivity and marvellous adaptability of youth and early manhood appear to depend upon the relatively great mobility of the cell-processes, while the retarded and restricted mental action so often seen in old age appears to be related to a much diminished mobility of these processes. Certain it is that the theory that the neuron is not an absolutely fixed morphological element renders clear and comprehensive many of the well-known facts of mental physiology. The application of this theory, for instance, to memory, sequence of thought, association of ideas, and even more complex mental processes, is evident, but the limits of this paper have forbidden any but a mere mention of them.

EXTRAUTERINE FŒTATION.

REPORT OF FIFTEEN CASES TREATED BY OPERATION.

By Andrew J. McCosh, M.D.,
of New York.

Within the past four years the following fifteen cases of extrauterine fœtation have been operated on by me at the Presbyterian Hospital. The admission of so many cases to the service of one surgeon in a general hospital must indicate that an ectopic pregnancy is a comparatively common occurrence. From the histories which follow certain facts have been derived. The youngest patient was twenty-three years of age, the oldest thirty-nine. Three were nulliparæ. Twelve had previously been normally pregnant, nine having borne one or more children, three having had miscarriages only. Five had complained of more or less pelvic pain prior to the ectopic gestation; ten gave no history of special distress in their pelvic organs.

Symptoms during the ectopic pregnancy. All had suffered from pain. In three the pain had been slight, in twelve severe. All had skipped one or more menstrual periods. In fourteen there had been irregular uterine bleeding; in one there was no history of bloody vaginal discharge. In eight there had been signs of shock on one or more occasions indicating rupture, in two of these amounting to severe collapse. In seven there had been apparently no sign of shock. Seven had considered themselves pregnant; eight had not suspected the existence of pregnancy.

Conditions found at operation. In two cases the fœtus was in the tube, which was unruptured (eight weeks and ten weeks). In five rupture into the broad ligament had occurred (less than three months). In eight the fœtus or its remains was found in the peritoneal cavity. The duration of the pregnancy was under fourteen weeks in thirteen cases, over four months in two cases (four and a half and six and a half months).

It may be well to note that most of these patients were from the lower grades of life and would not be apt to pay special attention to either their menstrual dates or their signs of pregnancy, and perhaps not to pain unless it was severe in character.

It will be noticed from a review of the above symptoms that these cases emphasize the fact that the main points in the diagnosis of extrauterine pregnancy are, first, delay of the menstrual period, with or head was deformed and both legs were crooked. It was not placed in an incubator, and died at the end of a few hours.

CASE XI.—K. K., aged thirty-two years; married eleven years. Never pregnant. With exception of an attack of pelvic pain five years before, no complaint of uterine or ovarian trouble until present illness. Last normal menstruation July 8, 1895. July 15th, seized with severe pain in pelvis which continued for two days. August 6th, bloody vaginal discharge for six days, accompanied by slight pain. August 20th, attack of severe pain and return of "flow." Pain and flow continued more or less constantly until her admission to hospital on September 12th. Pain described as cramp-like, mainly on right side. Temperature 100° to 100.5°; pulse 80 to 100. On examination moderate abdominal distention and a boggy, ill-defined mass was felt extending out of pelvis well above its brim and as high as the right anterior superior spine of ilium. Diagnosis—probable extrauterine pregnancy. Immediate operation advised, but refused. During following five days evening temperature ranged between 100° and 101°; pulse 100 to 120.

Operation, September 17th. Ether. Trendelenburg position. Median laparotomy. Intestines agglutinated to each other and to the mass, which, the size of an orange, occupied the right iliac fossa, and was attached to uterus and floor of pelvis, and apparently consisted of a distended broad ligament to which were adherent an enlarged tube and right ovary. Sac ruptured; an ounce or two of bloody fluid escaped. Tube and ovary removed. Cavity cleansed and in it was placed a gauze-strip, the other end projecting through abdominal wound, the remainder of which was closed by suture. Convalescence uneventful. Gauze removed September 21st. Patient out of bed October 16th. Discharged cured October 26th.

Report of Dr. Thacher, pathologist, states: "The Fallopian tube presents a dilatation one inch in diameter near its centre. This sac has ruptured and is filled with a solid blood-clot. Microscopical examination: Villi of the chorion."

CASE XII.—D. S., aged thirty-seven years; married; three children. Last normal menstruation September 6, 1895, after which no sign of bloody vaginal discharge until the day of her admission to hospital, November 6, 1895. On October 25th she was seized with a severe attack of abdominal pain, chiefly located in the left iliac fossa, which was followed by leucorrhœa. On October 30th another sudden attack of pain, followed by syncope and vomiting. Great prostration and some abdominal pain and distention followed and continued until the morning of November 6th, when she was seized with a third attack, in which the pain was agonizing, the patient losing consciousness and continuing in a condition of collapse until her admission to the hospital in the evening of that day. In the ambulance during transfer to hospital she had several attacks of syncope in spite of active stimulation. On admission her radial pulse was almost imperceptible. Abdomen considerably distended and tender. By vagina the pelvis was found filled with a boggy mass. Diagnosis—ruptured ectopic gestation. Collapse, however, was so great that operation was for the present out of the question, as she would surely have died on the operating-table. During the night she rallied somewhat and on the next afternoon her condition was such that a laparotomy was deemed justifiable, as further delay probably meant a fresh hemorrhage. Under ether-anæsthesia in Trendelenburg position, the abdomen was